

JOURNAL

of the

Society for Psychical Research

VOLUME 37 No. 678 January-February 1954

SERIALISM AND THE UNCONSCIOUS

BY G. F. DALTON

J. W. DUNNE's theory of Serialism, although it aroused much interest when first put forward, has not up to the present received general acceptance among psychologists or parapsychologists. The aim of the present paper is to show that, with certain minor amendments, the theory can be used to describe, and to account for, a wide range of psychological phenomena. In particular, it provides what may be called a geography of the unconscious, and the concept of 'repression' may be derived from it by deductive reasoning.

For a detailed statement of the theory, Dunne's own books must of course be consulted; but for present purposes it will be convenient to have a summary of its chief relevant features. These are: A. The regress of Time; B. The regress of consciousness; C. The combination of the two regresses; D. Intervention.

A. Time is conceived by the theory (and also in everyday life) as a length travelled over by a moving 'now'. The idea of movement involves both space and time; but since the 'space' concerned is ordinary time, the 'time' must be some other kind of time. Ordinary time—'Time 1'—is thus revealed as a fourth dimension of space, along which moves a three-dimensional field of presentation—the 'now'; the time in which this movement occurs being 'Time 2'. But 'Time 2' itself must similarly be regarded as a length, and the argument repeats itself *ad infinitum*. There is thus an infinite series of Times, and an infinite series of moving fields of presentation, each of which has one dimension more than the last.

B. A 'self-conscious observer' is one who is conscious of 'himself' observing. That is to say, he observes a 'self' which observes the external world. But this observation of the 'self' is still the act of a self-conscious observer. The observer must therefore be aware of a second 'self' which observes the first, and, for similar reasons, of a third which observes the second—and so on indefinitely.

C. Each of the 'selves' of the second regress is equated by Dunne to one of the 'nows' of the first regress. 'Observer 1', the 'self' which observes the external world, is nothing else but the three-dimensional field of presentation which moves along ordinary time (Time 1). Observer 2 is the four-dimensional field which moves along Time 2, and so on. (I shall in future abbreviate these terms to O₁, O₂, etc.)

D. The 'selves' are mere automata whose future careers are provisionally determined. Events which are in the future for O₁ are already present in the more fundamental 'now' of O₂. The only thing which can alter these events is an intervention by an entity described in Dunne's earlier books as 'the ultimate observer', but afterwards more vaguely as 'a higher-order observer'. It is to this entity alone that will and consciousness are attributed. It can intervene at the Time 1 'now', but not elsewhere, to change the otherwise determinate future. Consequently it normally concentrates its attention on O₁. This concentration becomes a habit which is very difficult to break by an act of will, because an act of will is normally associated with an intervention, and hence with an even stronger concentration of attention on O₁. Attention is relaxed in sleep, and partially in waking states of abstraction. Under these circumstances the four-dimensional field of O₂ is used. The attempt to fit four-dimensional events into the more familiar three-dimensional picture accounts to some extent for the fantastic nature of dreams.

Such is, in the baldest outline, the theory of Serialism as far as it is relevant to the present purpose. Undoubtedly at first sight it seems fantastic, but there exists a certain amount of evidence in its favour. At the same time there are valid objections to some parts of it. The evidence and the objections will be discussed under the same heads as have been used for the summary of the theory.

A. The most striking evidence of the existence of more than one kind of Time is the fact of precognition, which must now be regarded as established. Conformably with the theory, precognition is not under the control of the will, and takes place most readily in dreams or dreamlike states. The idea of precognition is inconceivable unless there are at least two different kinds of time. It does not follow, however, that they are related in the manner described by Dunne; another theory involving two Times has been tentatively suggested by Broad, for example.¹ The distinctive feature of Dunne's theory is that for any Dunne observer there is only one true Time, all inferior Times being seen

¹ *Philosophical Implications of Foreknowledge*. Aristotelian Society, Supplementary Vol. XVI, 1937.

as dimensions of space. To the dreaming mind, therefore, all Time 1 events are equally present. This receives some confirmation from such cases as that quoted by Havelock Ellis, in which the subject dreamed that she read a Shakespeare sonnet 'not line by line, but all at once—a weird sensation'.¹ An analogous musical experience is attributed to Mozart; unfortunately the letter on which this is based is almost certainly a fabrication. Dunne's theory accounts, too, for the peculiarities of dream time—a short nap may produce a disproportionately long dream; and I have myself had hypnagogic visions which were certainly momentary, yet contained action enough for a minute or more. It also suggests an explanation for the paradox that the *dénouement* of a dream story may be unknown to the dreamer. Formerly it had to be supposed that some part of the mind excogitated the story somewhere unknown to consciousness, and then presented it to the dreamer in its finished form. But if in dreams we think four-dimensionally, this hypothesis is unnecessary. The story is made up as a whole, and presented to consciousness as a whole; it is only on waking that the three-dimensional aspect is restored.

Vague statements about the abolition of time, or some profound modification of it, are common among mystics and mediums, but can hardly be put in evidence. Du Prel² quotes a statement by Luther that 'God sees time not lengthwise but crosswise', which may perhaps be claimed as an intuitive perception that Time 1 and Time 2 are at right angles. I have not been able to find this passage in Luther's works, and it may be as apocryphal as the Mozart letter, which Du Prel also quotes.

Mr Hanna, the celebrated total amnesic, had dreams in which he remembered his past life.³ On one occasion he passed into a state in which, without waking, he replied to questions. He was dreaming about events which had occurred years before at a place called Mount Jewett; asked if he knew Miss C., his fiancée, he laughed and said: 'I don't know her yet; I know her later. From her to Mount Jewett is a year'. One could scarcely have a clearer expression of Observer 2's viewpoint.

B. The early stages of the regress of consciousness are easily open to introspective observation. Daudet (quoted by James⁴) speaks of the 'second me' or 'second self,' which looks on while the first self acts and feels: 'this second me that I have never been

¹ 'Relation of Erotic Dreams to Vesical Dreams.' *J. Abn. Psychol.*, 1913, viii, 145.

² *Philosophy of Mysticism*, trans. Massey, 1889.

³ Sidis and Goodhart, *Multiple Personality*. 1905.

⁴ *Varieties of Religious Experience*, 1902.

able to intoxicate, to make shed tears, or put to sleep. And how it sees into things, and how it mocks!

'Homo duplex!' exclaims Daudet, but he should have said 'Homo triplex!' He has accurately noted the first and the second 'me', but has failed to note the 'I' which observes both the others and notes the contrast between them, *and which itself does not sleep or become intoxicated*, otherwise it would not be able to observe the second 'me' under these conditions. His remarks on intoxication are confirmed by McDougall, who says that the drinker 'leads a double life; his inner self, a detached observer with folded hands, watches his bodily actions, not seldom with surprise, consternation, or amusement'.¹ These emotions of the 'second self' are contemporaneous with the much more violent emotions of the intoxicated 'first self'; and the third entity, whose existence can be inferred as in Daudet's case, watches both the others without any emotion at all.

Selma Lagerlöf pushes her introspection still deeper. She speaks of the 'spirit of self-analysis' which 'sits and stares with its icy eyes. . . . And if you look closely, you see behind him another pale being who stares and sneers and paralyzes, and behind him another and another, all sneering at one another and the whole world'.² Here we have an indefinite number of 'me's' arranged one behind another, observing one another and the external world—as good an introspective description of the Dunne observer series as could reasonably be hoped for. The 'sneering' is, I think, largely an illusion of contrast; a spectator who looks on calmly at an emotional scene may easily be accused of sneering at it.

As in the last section, there are many vaguely relevant mystical or mediumistic statements. Perhaps the most noteworthy of the latter is that of Mrs Willett,³ who speaks of a 'chain of me's' through which the communications must pass.

In sleep the 'first self' is temporarily abolished, and consciousness transfers its attention to dreams. The dream is the work of a second component of the personality, the dream 'dramatist', and it is watched by a third component, the 'spectator'. There is often opposition of character and motive between the 'dramatist' and the waking personality⁴; the 'spectator' is much more like the latter, but is unemotional and detached.⁵

¹ R. Fortune. *The Mind in Sleep*, 1927, pp. 90 ff.

² *Outline of Abnormal Psychology*, 6th ed., 1948.

³ 'The Auction at Björne,' in *Gösta Berling's Saga*, 1918.

⁴ *Proc. S.P.R.*, 1935, xliii, p. 149.

⁵ G. F. Dalton. 'The Solution of Problems in Dreams.' *Journal S.P.R.*, xxxvi, 670-1.

In hypnosis, again, there is normally a triple division. The waking personality, as in sleep, is temporarily abolished. A second personality emerges which is highly suggestible, acts parts skilfully, and sometimes differs markedly in character from the waking personality. But behind this second personality, and apparently unknown to it, is another which watches its actions, and intervenes, if necessary, by putting an end to the hypnotic trance.¹ It is possible to split up the hypnotic personality further; Gurney, in several cases, produced two such fractions,² Mrs Sidgwick as many as eight,³ and McDougall⁴ believes that the process could be continued indefinitely. These fractional personalities are mutually amnesic, and (as far as is known) exhibit no striking variations of character.

Turning to psychopathology, we have the rare but striking cases of multiple personality. These may be divided into two groups: one characterised by mutual amnesia and little change of character, occurring chiefly among male subjects, and the other, almost confined to women, having a decided change of character and serial amnesia—each personality being conscious of those below it but not of those above. In cases of the second type, the characters are remarkably constant. The primary personality is melancholy, often ill, timid and morbidly conscientious; the secondary is gay, childish and irresponsible; the tertiary (present in much fewer cases) is grave, mature and altruistic. These tertiaries have the detached and watchful attitude which we have already noted in the corresponding entities in dreams and hypnosis. In the Doris Fischer case, for instance, 'Sleeping Margaret' conferred with Dr Prince quite after the manner of a consulting specialist. The secondary and tertiary personalities do not sleep, and (though there is less evidence on this point) are little affected by alcohol. In serial-amnesic cases there are scarcely ever more than three personalities, but in mutual-amnesic cases there may be as many as eight or nine.

There is thus what may be called overwhelming evidence that the human personality is divisible, and that the divisions are related in the manner required by Dunne's theory. But the Dunne observers are mere characterless 'abstractors' or 'now-marks', having no function beyond that of relaying sense-data in one direction and interventions in the other. The fractional personalities of real life are far more complex. They have characters, emotions, motives. They quarrel among themselves.

¹ W. McDougall, *op. cit.*, p. 91.

² *Proc. S.P.R.*, iv, 515.

³ F. W. H. Myers. *Human Personality*, 1903, i, pp. 452-5

⁴ *Op. cit.*, p. 99.

Clearly the theory will require much elaboration in order to deal with this discrepancy.

C. The only evidence of a connexion between the observer regress and the Time regress is that of dreams. The dream 'dramatist' perceives four-dimensionally, as is shown by precognition effects ; and the 'dramatist' is also the second term of a regress of consciousness. (The dream 'spectator', O₃, should perceive five-dimensionally ; but it would be an almost impossible task to find any evidence of this.)

But this evidence of association is not evidence of identity. The successive 'nows' of the Time regress are physical entities, travelling through space with a definite velocity. The successive 'selves', on the other hand, are psychical entities. To identify any 'self' with its 'now' is to beg the whole question of mind and matter. I must conclude, therefore, in disagreement with Dunne, that there is both a physical and a psychical regress, each 'self' being associated with the corresponding 'now' in a manner which, like the general problem of psycho-physical association, remains a mystery. The fact of association, however, is all that is required for the further development of the theory.

D. It is of course impossible to *prove* by evidence that the future can be changed by intervention, but there are several spontaneous cases in the literature which strongly suggest that this is so. Thus a woman dreams of her toe being cut off as she goes down the stairs ; next morning, going down the stairs with bare feet, she remembers her dream, looks carefully, and sees a knife lying edge upwards just where she would have stepped on it.¹ Another dreams of being burnt alive, and leaves her hotel in consequence ; next day the hotel is destroyed by fire.² There are other similar cases, but their number is small compared to the total number of precognitive dreams. This is not surprising. Not many people will be influenced by a dream to the point of taking action, and if action is taken it may be too successful, and leave no evidence behind. If, for instance, I cancel an intended motor-car journey on account of a dream of a road accident, there will be nothing to show whether the accident would have taken place.

Intervention is not, of course, limited to action taken in consequence of precognitive dreams ; but without such a dream there cannot be any direct evidence of intervention.

In the earlier editions of *An Experiment with Time* the power of intervention was attributed to 'the observer at infinity' ; but it

¹ *Proc. S.P.R.*, viii, 391.

² W. O. Stevens, *The Mystery of Dreams* (London, Allen & Unwin, 1950), p. 132.

was objected by Broad¹ that 'the observer at infinity' is 'the last term of a regress which, by hypothesis, *cannot have* a last term', and therefore 'the notion of the observer at infinity' is a self-contradictory notion and there is no such observer. It would perhaps be clearer to say that there can be nothing *distinctive* about the observer at infinity; he cannot have any properties which are not possessed in an almost equal degree by the observer at 'infinity minus one' (to commit a mathematical barbarism). Dunne considered that he had met this objection by substituting the expression 'higher-order observer'; but the criticism still applies. Once the first term of the regress is passed, we cannot pick and choose among the terms. We cannot attribute intervention to one higher-order observer and deny it to another. If intervention occurs at all, it must be possible for any observer except the first to intervene. There are several ways in which this might occur; but the hypothesis most in keeping with the general character of the regress is that intervention is serial. That is to say, O₂ may intervene to alter the otherwise determinate career of O₁; but O₂'s intervention will itself appear to O₃ as determined and foreseeable, and O₃ can interfere with O₂'s intervention. The same applies all the way up to infinity.

These interventions by an O₂ or O₃ cannot be considered as acts of will; they are mere automatic reactions. Will and consciousness remain unaccounted for. I do not wish to pursue this question further at present, since it raises far-reaching and complex questions which cannot be dealt with in a purely preliminary survey. My object at present is simply to show that the theory of Serialism has sufficient *prima facie* reasonableness to make it worth considering as a working hypothesis. If the reader will grant this, we can proceed to use it as such. Let us consider the first few terms of the regress—say the first three, as a triple division of the personality seems to have some special importance—and let us examine the consequences of an intervention which originates at or beyond the third term.

In the absence of intervention, the behaviour of the subject is in accordance with the character of O₁, and is fully determined; in a given situation the same stimulus will always evoke from O₁ the same response. To interfere with this automaton requires the exertion of psychical and probably of physical force. This is an intervention, and as a result O₁ acts out of character; it takes on for the time being the character of the intervening entity. The inertia of O₁ is the resistance against which the force is exerted; and, of course, whatever exerts the force will feel the

¹ 'Mr Dunne's Theory of Time.' *Philosophy*, 1935, x.

resistance. (This is the foundation of the theological conception of conscience.)

If the intervention is repeated every time the appropriate situation recurs, the resistance decreases, and in course of time vanishes. The new mode of behaviour has become part of the structure of O₁. If the reader will permit me to reintroduce the idea of the will, ignoring the logical difficulties which it presents, we can put all this much more simply : an effort of the will, if long enough persisted in, will form a habit.

The change which takes place is not merely physical, but extends also to the emotions ; for if behaviour is systematically modified by any means, emotions will be modified to suit. A child who is taught to behave respectfully towards his teacher will come to feel respect for him ; a soldier who is trained to behave as if he were not afraid will feel less fear. James's arguments on this point are well known. The change in the emotions is a proof that it is the psychical entity O₁ which has been modified, and not merely some physical network of synapses.

So far only two terms have been considered ; but the postulated intervention originates at or beyond the third term. It is implied in this that O₂ does not intervene independently. Since it has the power to do so (if we accept the hypothesis of serial intervention), its inaction must mean that it has no motive for intervention. Its character, therefore, must be in harmony with the unmodified O₁. The intervention of O₃ must (by the general logic of the regress) take place via O₂, and not directly on O₁. O₂, therefore, must, like O₁, be forced to act out of character ; it will resist this, and O₃ will be aware of the resistance. So far the effect of the intervention on O₂ has been similar to its effect on O₁, and we might expect that, as with O₁, continued intervention will permanently change O₂'s character and remove the resistance.

But this cannot happen. O₂ is associated with a four-dimensional field, and intervention is a three-dimensional process, occurring at a particular moment of Time 1. It is clear that intervention cannot modify the whole field of O₂. In particular, the Time 1 'past', which is still present to O₂, is not accessible to intervention.

We do not know the precise nature of the association between a Dunne observer and its field, but it is certainly a close one. Now, if there occurs a sustained intervention which greatly affects the character of O₁, O₂'s field will consist of two widely differing parts. It does not seem unreasonable to suppose that this will be reflected in the character of O₂ itself ; that it will be split into two opposed, co-existing entities. One of these, which may be denoted O_{2b}, is in harmony with the character of the intervening

O₃ and the new O₁ ; the other, O_{2a}, is similar in character to the old O₁, and hence is opposed to O₃. O₃ can never get rid of this opposition ; it must always act against it, and hence must be aware of it.

But O₂ is, of course, the entity responsible for dreams. If the hypothesis is correct, we should expect to find in dreams some evidence of the O_{2a} character. That is, some dreams should show an interest in events of the pre-intervention period, opposition to the waking personality, and a reversion in character to that of the early period.

Now an alteration of behaviour of a very thorough nature has taken place in the life of almost every adult through education, and in particular through training by the parents in early childhood. It may seem curious to describe this alteration as due to intervention, since it is in the first place due to the parents and not to any endo-psychic entity. But unless such an entity exists, education is impossible ; no completely external force can *compel* action. Education consists in the creation of an artificial environment, to which the child must respond with certain alterations of behaviour, considered to be socially desirable. But only the child himself can actually effect these alterations ; and in so doing he must make an effort of will, i.e. an intervention.

Here, then, we have the sustained intervention which has been postulated. The results are fully in accordance with the theory. As everyone knows, dreams do in fact frequently show an infantile character, interest in events of early life, and motives opposed to those of the waking personality. It is needless to quote examples. But it may be argued that O_{2b}, as well as O_{2a}, is a denizen of the O₂ level, and equally competent to produce dreams ; there should therefore be dreams which show the opposite characteristics. This is quite correct ; there are many dreams which show an adult and helpful attitude towards contemporary events. The most striking are those which solve a problem on which the waking personality has been concentrating.¹

Some of the leading characteristics of dreams are thus accounted for, but others will require further examination. One of the most striking is the use of indirect and symbolic allusions, to account for which Freud put forward the theory of a 'censorship'. In order to explain this, we must first consider the nature of sleep and its effect on the Dunne observers. Dunne expressed the view² that in sleep, generally speaking, the O₁ term ceases to exist, i.e. that contemporaneous sensory stimuli are only intermittently observable by the dreamer. But there is much evidence to the

¹ Dalton, op. cit.

² *An Experiment with Time*, p. 192.

contrary ; indeed, the point can be proved from one of Dunne's own dreams, which was brought about by the stopping of a watch. How can the *cessation* of a watch's tick cause a dream unless some part of the personality remains continuously awake to listen to it? Moreover, the same example shows that the sense of hearing, and presumably the other senses also, are not in the least dulled. It does not need a loud sound to wake the sleeper, provided that it is significant. Rivers takes the case of the doctor who wakes at the sound of the night-bell, while his wife sleeps on, but is herself awakened by the first cry of her baby.¹ Other examples are given by Freud.² We are therefore forced to conclude that messages from the sense-organs are being continuously presented to some part of the personality which judges them intelligently, and wakes the sleeper when this is considered necessary. The only entity capable of doing this is O₃, or some higher term. We have already seen that an O₃ entity in hypnosis performs a similar function.

The stimuli are presented to O₂ as well as O₃, for they are frequently worked into dreams. Dunne's case is an example of this, and Horton³ and Havelock Ellis⁴ have sufficiently elaborated the point. It will then naturally be asked : if all the higher observers are observing O₁, how does sleep differ from waking? The answer is that the sensory stimuli, though present in the fields of O₂ and O₃, are normally not attended to. To attend to such stimuli is, in fact, to be awake. In sleep O₃ relaxes its attention, and follows the wanderings of O₂ over its four-dimensional field ; at the same time it is marginally aware of O₁, and if anything significant happens there, it refocusses its attention, and so wakes.

This is all that is necessary for the operation of the 'censorship'. Sleep can be maintained only on the condition that no strong emotion is aroused ; for emotion pertains mainly to O₁, attention to strong emotion is attention to O₁, and attention to O₁ is waking. But O_{2a} tends to direct attention towards past emotional experiences. If it does so too directly, it becomes aware of O₃'s refocussing attention. This is not to say that it becomes aware of O₃, which is contrary to the whole theory ; but O₃ must intervene via O₂, against the resistance of O_{2a}, and it is this interventional thrust which is felt by O_{2a}.⁵ The struggle is resolved by a com-

¹ *Conflict and Dream*. London, Kegan Paul, 1932.

² *Interpretation of Dreams*.

³ A series of papers in *J. Abn. Psychol.*, xiii-xv.

⁴ *The World of Dreams*. London, Constable, 1911.

⁵ We might perhaps use Sally Beauchamp's term, and say that O_{2a} feels 'squeezed'.

promise ; O2a keeps away from the central point of interest, but wanders all around its periphery.

It may be noted in passing that we have not to account for the occurrence of dreams in general. For the Serialist, there is no such thing as unconsciousness ; whenever the attention is diverted from the three-dimensional world, whether by drowsiness, drugs, anaesthetics, concussion, or anything else, it must be diverted towards some other field, and thus produce a dream—though the dream may not always be remembered.

In the foregoing analysis O3 has been treated as equivalent to the will. This is, of course, only a first approximation. A closer examination would show the existence on the O3 level of an autonomous entity, the Freudian Super-ego, which is not the will, and may be opposed to it. The Freudian Id is obviously O2a (he almost ignores O2b phenomena) ; and O1, the 'poor Jackself' trying to serve two masters and at the same time buffeted by the external world, is the Ego. The last entity is the only one of which we are normally conscious.

REVIEWS

PSYCHOANALYSIS AND THE OCCULT. Edited by George Devereux.

New York, International Universities Press, 1953. xv, 432 pp. \$7.50.

This is a notable book, and will inevitably become a landmark in the history of research. For parapsychologists it is important in that it gives for the first time, in one comprehensive volume, almost the entire thought of psychoanalysts on telepathy since Freud's first paper on this subject in 1899. For psychoanalysts, the publication of this book is equally important, suggesting as it does the beginning of a new epoch, in which the problem of telepathy has become crucial for them. It may be difficult reading for any but the analytically trained, but it is essential that parapsychologists should give it a prominent place in their studies.

The use of the word 'occult' in the title may lead to disappointment for those readers for whom the word very naturally connotes a very wide range of phenomena. Here, only telepathy is under examination. This limited use of the word 'occult' may derive from a recommendation made by Freud in his study 'Dreams and the Occult', where he writes : 'Among these conjectures the most probable is, I think, that in occultism there is a core of facts which have hitherto not been recognized, and round which fraud and fantasy have woven a veil which it is hard to penetrate' and suggests that '... we should pick out the theme of telepathy from all

the confused material that surrounds it.' (p. 95). The reader will also find that telepathy or thought transference (the words are often used interchangeably throughout the book) is examined only in the limited field of the analytic interview. Dr Devereux states that 'The task of the analyst in the therapeutic setting is exclusively to analyze in order to cure.' (p. 417). Direct research, therefore, has not been feasible, but in spite of this the investigator of psi phenomena will discover here observations and insights which are of great interest, and which are indispensable to him in his own researches.

Dr Devereux, who is the editor of this anthology, is Director of Research at Winter Veterans Administration Hospital and Lecturer in the Menninger School of Psychiatry. He is the author of *Reality and Dream: Psychotherapy of a Plains Indian* and of numerous scientific papers. It would be hard to better the way in which he has edited this volume. He states clearly his neutral attitude to psi phenomena—in fact he says later that he is a 'hard-boiled sceptic' but the collection of writings here shows good evidence of his objectivity. It is evident that there is no official attitude to telepathy; the views expressed vary from whole-hearted acceptance, and all which that acceptance might entail, to a complete denial or a verdict of 'not proven'.

The collection of writings is comprehensive, containing all but one of the papers which represent clinical psychoanalytical contributions to the problem of so-called psi phenomena. The one exception is István Hollós's paper, which is recognized by both the advocates and the opponents of psi phenomena as one of the most important inquiries into this much-vexed problem; it was unfortunately not possible to get permission to print this paper; the editor states therefore that he 'attempted to fill the gap, in part at least, with a brief and non-controversial presentation of Hollós's views'. Many readers will feel an especial regret that there is no paper from Dr Ehrenwald, whose hypothesis of the minus-function has created such wide interest, but we are told that his best papers are, or soon will be, available in book form, and were therefore not included.

Part I. Historical and Methodological Surveys.

In Chapter 1 Dr Jule Eisenbud in an interesting paper 'Psychiatric Contributions to Parapsychology: A Review' gives a summary of research through Janet, Richet, Stekel, Freud, and Hollós; then mentions the modern workers, Rhine, Schmeidler, Humphrey, and Ehrenwald. He emphasizes the immense importance of collaboration between parapsychologists and psychiatrists.

In Chapter 2, 'Extrasensory Perception and Psychoanalytic Epistemology', Dr Devereux suggests that an interest in psi phenomena derives from archaic interests and wishes and that these, however regrettable, will form the motive force driving us to find a truly scientific explanation for 'this turbulent cluster of facts.'

Dr Devereux's view is that the most 'parsimonious' explanation of telepathy can be shown to lie within the analytical framework, and illustrates this with a hypothetical case. As the discussion of this case is of great theoretical interest, we will summarise it here.

Dr Devereux states that 'Psi phenomena are psychological phenomena which must be studied in the psychological frame of reference', and goes on to ask, 'Precisely what is a psi phenomenon?' The hypothetical case is as follows: Let us suppose that at 8 p.m. John Doe's Chevrolet collides with a Ford on the corner of 10th Avenue and 34th Street in New York. Also at 8 p.m., Mrs John Doe, in Boston, exclaims: 'My husband's car has just collided with a Ford on the corner of 10th Avenue and 34th Street.' (It is assumed that this is most thoroughly witnessed and recorded on a tape machine.) These are two discrete events; we must not call them a 'pair' of events, for we are not yet dealing with psi phenomena. Dr Devereux then goes on to introduce his theory. He states that as analysts firmly believe in psychic determinism it makes no difference, from the viewpoint of the problem of the existence or non-existence of telepathy, whether it is the manifest or latent content of the utterance, dream or vision which is congruent with the thought of the sender or receiver or with some event in the external world.

There is no need to invoke a telepathy hypothesis if Mr and Mrs Doe are unhappily married. Mrs Doe's utterance was prompted by a death wish towards Mr Doe; the details and numbers she expressed were derived from her past life and conflicts (which would be revealed in analysis). Mr Doe's accident was caused by a self-destructive tendency. In other words, events in both cases could be accounted for by inevitable psychic determinism. The events are discrete, at least in terms of simultaneous causation. They are connected only in terms of the past by means of antecedent causation within a psychoanalytic frame of reference. This is an important problem; Professor H. H. Price, in a communication to the reviewer, has suggested an answer as follows.

I don't think that Dr Devereux's argument is valid. Taking the author's example, the theory he has to explain is that Mrs Doe makes the observation *at a particular time*—the time when the accident is happening. Let us suppose that Mrs Doe does have this death wish towards Mr Doe. This is a relatively permanent fact about her. She

has had this unconscious wish for many months or for many years. Similarly, the numbers, etc., have had this emotional significance for her for a long time, even since her infancy perhaps, when the relevant conflict was first set up ; and all this, however true it is, does not explain why she makes her statement *now*, just at the crucial moment when the accident is happening. The fact that she makes it *at that particular hour* is, of course, just the fact which interests the psychical researcher. So far as the death wish and the emotional significance of the numbers are concerned, she might equally well have made this statement at any time during a very long period. The believer in telepathy can perfectly well argue that her utterance was determined—in fact he must maintain that it is so. What Dr Devereux is maintaining is not just determinism in general, but a special form of it—what one might call endo-psychic determinism: the theory that all mental events in X's history are determined by previous events *in X himself*. I suppose he would have to make an exception when the events in question are sensations or perceptions caused by external physical stimuli. But it looks as if he would hold that all mental events other than these are 'endopsychically' determined. Of course, he can propound this as a hypothesis, if he likes. But then we must ask him to prove it, and in order to do so, he will have to produce a convincing 'endopsychic' explanation of the phenomena (e.g. apparently telepathic ones) which suggests that some events in X's mental history, other than sensations or perceptions, are *not* determined by previous events in X's mental history, or not wholly, but by events in the mind of someone else, Y. This explanation he has failed to produce, if my criticisms above are right.

Incidentally, the point about Mr Doe's accident being caused by a suicide tendency is not very convincing. No doubt some accidents are so caused. But if we say that *all* are, what explanations are we to give if some taxi driver skidded on a patch of oil and collided with Mr Doe and killed him? I think we shall have to say that Mr Doe had an (unconscious) precognition of the taxi driver's skid, and therefore (unconsciously) arranged that he himself should be at just the appropriate place at just the appropriate moment. At any rate, some sort of conscious ESP on Mr Doe's part will have to be postulated—in order to save a theory which purports to abolish ESP! Similarly, to change the example, a suicide tendency itself will not explain why Mr A travelled by a train which subsequently had an accident in which he got killed—unless one supposes, in addition, that he had a precognition of the accident (unconscious, presumably) or a clairvoyant cognition of a damaged rail or bridge or whatever it was that was going to cause the train to be derailed. Finally, I have an uneasy feeling that even if Mr Doe's car had collided with a bus on the corner of 5th Avenue and 19th Street, and Mrs Doe's statement had corresponded with this, an equally convincing or unconvincing explanation could have been produced of why buses and the numerals 5 and 19 had a special emotional significance for her. This kind of explanation is too easy! Almost any familiar object and almost any numeral could probably be shown to

have some emotional significance and to be associated with some 'conflict' or other by a sufficiently ingenious psychoanalyst.

I do not think the author's theory would appear even plausible in experimental cases when A repeatedly gets hits coinciding with all sorts of different thoughts in B. It would be weakened also if Mrs Doe repeatedly made veridical statements about different people, including some people not known to her.

Dr Devereux then discusses ESP and theories of causation and precognition. He writes: 'An entirely different situation arises if one believes from the outset in ESP, including precognition, and re-examines the problem of causality in the light of this belief. At this point the entire problem imperceptibly fuses with the problem of the status of the causality principle in science.' (p. 31).

He suggests a theory where '... causality and psychic determinism of a historical kind are replaced by, or are identical with, a type of structuring of perception. Cause and effect are simultaneously present. All perception (including ESP and prophecy) is then of one type only, and concomitant "perception" alone decides whether two phenomena are viewed as causally or structurally connected, or else as discrete events.' (p. 32).

Part II.

In Part II we have six studies by Freud, one translated into English for the first time. It is invaluable for the student to have all Freud's published observations collected in this way, and to be able to follow his thought and his increasing belief in telepathy from his first paper in 1899 to the last in 1933. The papers are as follows: 'A Premonitory Dream Fulfilled', 'Premonitions and Chance' (an excerpt), 'Psychoanalysis and Telepathy', 'Dreams and Telepathy', 'The Occult Significance of Dreams', and 'Dreams and the Occult'.

It is hardly possible, and perhaps soon it will be hardly necessary, to emphasise the great importance of the insights given in these studies. Freud asserts that it is feasible for psychoanalysis to unmask a possible telepathic event which otherwise would never have been recognized as such. If telepathy were a fact, he concludes, then the laws of unconscious mental life could be taken for granted as applying to data telepathically perceived. As Dr Eisenbud says: 'With this sweeping, brilliant generalization, Freud marked the path for a whole new development in the investigation of psi phenomena.'

Part III.

In Part III, 'Studies by Psychoanalytic Pioneers', are papers by eight distinguished psychoanalysts: Edward Hitschmann, Helene

Deutsch, Géza Róheim, Fanny Hann-Kende, Hans Zulliger, Dorothy T. Burlingham, and Leon J. Saul.

It is not possible, in a limited space, to do justice to the interesting contributions from these writers, or to quote the cases which illustrate their views. The non-psychoanalytic reader may feel somewhat confused as he gropes his way down the echoing corridors of psychoanalytic associations and interpretations, but he will also learn much of great value. Dorothy Burlingham reports an interesting case of telepathy between mother and child, which suggests that this relationship is favourable to thought transference. Hans Zulliger gives cases where dreams, which might seem to be precognitive, prove to be the fulfilment of unconscious intentions on the part of the dreamer. Edward Hitschmann studies one case, where a son has a dream prophesying the death of his father (with an hallucination of the smell of his father's tobacco and a voice forecasting the death), which proved to be a projection of the son's unconscious death wishes with regard to his father, who had caused him great unhappiness. Hitschmann considers that a verdict of telepathy or not-telepathy is decided solely by one's subjective attitudes, mystical or anti-mystical tendencies, and says that 'Only a large number of such happenings will make it possible to settle the problem of telepathic incidents during analysis, which seems to provide an especially favorable setting for the occurrence of such incidents.' (p. 132). Leon Saul, in 'Telepathic Sensitiveness as a Neurotic Symptom', gives the case of a girl who claimed telepathic powers, but these were revealed by analysis to be due to hypersensitiveness to the emotional states of others.

Part IV.

In Part IV we have the Hollós-Schilder-Servadio Controversy. Here Dr Devereux gives a summary of Hollós's views. Hollós observed over 500 cases and took notes over many years. He considers that 'Since telepathic phenomena occur in conformity with certain laws, and under conditions susceptible of being specified, the possibility that they may be due to chance is automatically eliminated.' (p. 200). He goes on to say that 'It is precisely the patient's repression, conjoined with the analyst's freely receptive unconscious, which creates a condition suitable for communication between the analyst's and the patient's unconscious spheres.' (p. 203). Paul Schilder, amongst other criticisms of Hollós's findings, writes that if the idea of *symbolic* transmission, however remote, between agent and percipient is allowed, 'then one opens all doors to complete arbitrariness'. Servadio, who

writes very interestingly about telepathic hallucinations sums up : 'In my opinion no form of telepathic transmission—not even experimental telepathy—can come into being solely by means of pure, logical, rational and conscious thought . . . only psychoanalysis can provide us with an understanding of phenomena which impress us at first as utterly illogical, irrational and unconscious.' (p. 220).

Part V.

In Part V we have the Eisenbud-Pederson-Krag-Fodor-Ellis Controversy ; here the battle continues, with some heat, with Alfred Ellis as critic.

The papers contained in this controversy are Jule Eisenbud : 'Telepathy and Problems of Psychoanalysis', 'The Dreams of Two Patients in Analysis as a Telepathic Rêve à Deux' ; Geraldine Pederson-Krag : 'Telepathy and Repression'.

We notice here Dr Eisenbud's gift for clearly conjuring up the importance of the study of the 'occult', with all the possibilities of change and development which is implicit in it ; and Dr Eisenbud is among the few in this book to give due significance and real approval to the modern statistical approach. His own approach is, perforce, an analytical and pragmatistical one, wherein the ever-present possibility of coincidence cannot be guarded against. He reports a few occasions when he experimented, with numbers as material, for telepathic transmission. A three-figure number was chosen at random, but we are not told by what method ; the numbers which appear on the agent's or percipient's records are added to, subtracted or reversed ; and though Dr Eisenbud may claim legitimacy or plausibility for these changes (as they were made within a framework of accepted analytical conceptions), he cannot expect such experiments to convert the sceptical. The case is difficult to follow, being long and complex.

Dr Geraldine Pederson-Krag illustrates in the cases she cites the often aggressive nature of telepathy. She suggests that human speech had to be invented because telepathic communication revealed too much of man's aggressive and erotic impulses.

Dr Nándor Fodor's cases concern shared dreams. 'The recognition that a dream has a purpose and some ability to detect the purpose would be essential to the recognition of telepathic dreaming.' (p. 293).

Albert Ellis makes two stimulating contributions : 'Telepathy and Psychoanalysis : A Critique of Recent "Findings" ' and then 'Comments on the Discussants' Remarks'. He criticises, in detail, the interpretations and conclusions reached by the three other writers

in this controversy. Many of his criticisms seem impossible to refute and he makes the valuable suggestion that, amongst other safeguards, all the sessions of an analyst who is experimenting with these phenomena should be recorded by phonograph or sound film. He considers that uncontrolled research techniques and observational methods were employed in the studies reported, and that the hypothesis of telepathic occurrence is only one of several possible hypotheses. No truly scientific evidence has been offered, wherein we must agree. This critical spirit is welcome—but surely too sweeping when he says that he can find, in the organized research into telepathy done by the S.P.R., no significant controlled research during 75 years.

Part VI. New Contributions.

Dr W. H. Gillespie, the President of the British Psychoanalytic Society, contributes three interesting dreams, suggesting that they show a telepathic element. One dream he reports because it best demonstrates the point he is trying to make, that attention to extrasensory elements may be essential to adequate dream interpretation. He suggests that 'The facts of telepathy open up quite new possibilities of a much more direct means of communication between one unconscious and another', and that the parent-child relationship should be carefully considered from this angle (p. 381). He considers that the so-called delusions of schizophrenia, ideas of passivity and of influence etc., are based on a core of actual telepathic experiences. Sidney Rubin contributes a case, 'A Possible Telepathic Experience during Analysis'.

Part VII.

In Part VII, 'Extrasensory Perception and Psychoanalytic Technique', Dr Devereux makes a comprehensive review of the whole problem. This final summary will most appeal to those interested primarily in the technique of analysis, but some points arise which have not received attention from the contributors. An assumption is usually made that in telepathy a process of 'sending and receiving' is involved. Dr Devereux points out that the passive definition of reception is by no means the only possible one. He suggests that a passive, receptive attitude of the telepathetically sensitive patient conceals active voyeuristic tendencies such as spying. (At this point we begin to wonder whether Dr Devereux is really as sceptical as he declares.)

It is difficult to summarise a collection such as this, where there is a mass of material combined with many points of view, and which

is so rich in constructive observations and insights. A few comments and questions may be made, perhaps, from the point of view of a psychical researcher. We note the confusion introduced by the use of the word 'telepathy' with its assumptions of sending and receiving. But the expressions 'psi' or 'ESP' had not been coined when the majority of these articles were written. We should have liked information as to the reporting of dreams, in analysis, by whom and when. Considerable evidence is given of the aggressive elements in telepathy and its relation to repression. Is telepathy (ESP or psi) never used in a constructive rather than a destructive way? We can find no discussion of this important question.

If it is the statistical investigation of psi which has placed it on the map of Science, psychoanalysis has already made great contributions to the study of the 'occult' and can already show practical results. It can reveal as telepathic an incident which would otherwise go undetected as such. It can also show the normality of an incident which would otherwise be called telepathic. This is a major advance in research. But Dr Devereux makes no extravagant claims. He writes: 'The anthology is not so much a presentation of results as an invitation to hard work', and we cannot improve on Dr Eisenbud's summary of the situation made in his Review: 'Continued work in parapsychology from the psychiatric point of view and continued work in psychiatry from the parapsychological point of view can only prove mutually helpful. It is conceivable, moreover, that such cross-fertilization might even prove mutually revolutionary.'

I. JEPHSON

SOME ASPECTS OF THE CONFLICT BETWEEN SCIENCE AND RELIGION.

(The A. S. Eddington Memorial Lecture, 1953.) By H.H.

Price. Cambridge University Press, 1953, v, 54 pp. 3s. 6d.

Professor Price outlines the plan of his lecture as follows. 'I shall first try to show that the crucial issue in this long-standing controversy is nothing more or less than human personality. Two apparently irreconcilable answers are offered to the old question 'What is man?' I shall then suggest, in the last section of the lecture, that psychical research is the one line of inquiry which seems likely to throw fresh light on the dispute.' (p. 1).

Professor Price goes on to review in an impartial manner the respective claims of the religions and scientific outlooks, by no means underestimating the strength of the Materialistic theory. 'But', he continues, 'the empirical facts which favour the Materialistic conception of human personality are not the only ones which

are relevant to the issue. 'There are also the queer and disconcerting facts discovered by psychical researchers' (p. 34). This is followed by an appreciation of the present position in psychical research and of its bearing on the subject of the lecture. Readers of the *Journal* are already aware that Professor Price never offers anything on this subject without making some stimulating observations inviting further inquiry. They will not be disappointed here. Considerations of space permit me to quote only one such observation, or, rather, tentative hypothesis, which it might be possible to test by suitably devised experiments. 'There seems to be some barrier or censorship or repressive mechanism which tends to prevent supernormally acquired information from getting through into consciousness. It is quite likely that many telepathic (and clairvoyant) "impressions" never succeed in getting into consciousness at all; and we have to envisage the possibility that telepathy is going on all the time in some people, or perhaps in everyone, though the results only show themselves in consciousness occasionally.' (p. 38).

This lecture marks a bold advance in the movement to bring the findings of psychical research to the notice of orthodox religion and science, fields from which those findings, owing to their disconcerting character, still tend to be excluded by some sort of 'barrier or censorship or repressive mechanism'.

G. W. L.

THE JOURNAL OF PARAPSYCHOLOGY. Volume 17, Number 3, September 1953. Durham, N.C., Duke University Press. \$1.50.

In an editorial on 'The Prospect for Further Exploration' Professor Rhine discusses present prospects for parapsychology.

Dr Osis and Mrs Foster report a further 'Test of ESP in Cats'. In these experiments the cats showed a significant difference between the score in ESP tasks when the conditions were made pleasant for the cat and when they were made unpleasant, the cats scoring (as predicted) positively in the former case and negatively in the latter.

Mrs Rhine publishes a further contribution to her study of spontaneous cases under the title: 'The Relation of Experience to Associated Event in Spontaneous ESP'. The type of ESP experience with which this study is concerned is that of veridical hallucinations of calling.

'An Investigation of Extrasensory Perception in School Children' is reported by J. G. Van Busschbach. Although the rate of ESP scoring was low (a little over one per cent) with the number of trials made the results were significant.

There is a useful review of Kahn's *Studies in Extrasensory Perception* by J. G. Pratt and some valuable comments by J. Whittlesey on the discussion between Mundle and Flew on the meaning of 'cause' and 'effect'.

R. H. THOULESS

REVUE MÉTAPHYSIQUE. Paris, Institut Métapsychique International. 200 fr.

No. 25, September–October 1953.

The first article in this number consists of extracts from a thesis for his doctorate of medicine by Dr Hubert Larcher, on the alleged miraculous phenomena associated with the blood of certain deceased saintly persons, considered from the biological point of view. For some of these he suggests normal explanations, but others, notably the liquefaction and increase and decrease in weight and volume of the substance contained in a hermetically sealed phial at Naples, and traditionally said to be the blood of Bishop Janvier, have not, in his view, as yet been chemically accounted for. And in this case he considers fraud to be most improbable.

Dr de Cazeneuve gives an account of the cure of the King's Evil (scrofula) throughout the centuries, attributing this to the power of suggestion and faith.

In the third of a series of articles on radiesthesia as practised in France today, Jean Tenaille describes the methods of Jean Auscher, and, in an article entitled 'Radiesthesia and Medicine', Dr Jean Jarracot discusses the claims of Auscher and others that their various methods are an advance on normal medicine. Although he finds these excessive and unsubstantiated, there remains, in his view, a residue of phenomena worthy of serious investigation.

The review also contains translations of Professor Hardy's article on 'Biology and Psychical Research' in S.P.R. *Proceedings* and of one on 'The Psychic Fifth Dimension' in the A.S.P.R. *Journal*.

No. 26, November–December 1953.

In the November–December issue, Dr Hubert Larcher continues his study of the reputed blood of St Janvier at Naples. The phenomena connected with it, he points out, are analogous to those reported in the experimental researches of the Alchemists. He puts forward a speculative hypothesis to account for them, discusses its biological implications, if correct, and pleads for further study of the phenomena themselves.

Dr Mouzy-Eon describes the painful diseases suffered by Pascal

throughout his life, including a fantastic account by Pascal's niece of a spell said to have been laid upon him as a child, and discusses the possible effect on his creative work.

M. Raphaël Kherumian contributes an interesting article, with illustrations, on some telepathic experiments conducted by himself and M. René Hardy, with mechanical aids, a metronome and intermittent lights. He suggests that such rhythmic stimuli may help agent and percipient to synchronise 'the specific periodicity inherent in the functional processes of every individual', but says that the experiments were too few in number to give conclusive results. He reports them in the hope that further experiments may be conducted along these lines.

M. Edmond Offenstadt reports the studies carried out by Dr Manfred Curry at the American Bioclimatic Research Institute at Riederau-Ammensee near Munich, on the effect of atmospheric conditions on man and his health, and on the effects, in particular, of a certain form of ozone which Dr Curry claimed to have discovered. M. Offenstadt notes that Dr Curry assembled a large number of facts, but thinks it too early as yet to assess his conclusions.

M. Serge Hutin comments on the further researches made by Mr G. W. Lambert on the Trianon case, and speculates as to its cause.

R. I. H.

CORRESPONDENCE

PROBABILITY AND PARAPSYCHOLOGY

SIR,—In the November-December issue of the *Journal* Mr Spencer Brown states that my first significant results with Mr Shackleton were obtained by means of a series of sixteen cross-checks and that four of these gave significant results. 'There might', he says, 'be some justification for excluding these results from the field covered by the term "cross-check" if in the next experiment they had been repeated. Here, however, significant matching was found in only one of the positions tried in the former series.'

Now it appears to me that Mr Brown is taking much too narrow and rigid a view of the repeatability of a biological experiment. In a hundred years' time a physicist can verify, say, Ohm's law, because the relevant conditions of the experiment are practically unaffected by lapse of time, whereas organisms change with time. A person who shows a certain set of reactions to, say, tests in brain co-ordination will not necessarily produce precisely the same reactions in even six months' time. He may, for instance, while

seeming the same man to outward observation, be now in the incipient stage of locomotor ataxy.

Let me try to clarify the issues in the case of Shackleton. In 1939, at the request of Whately Carington, I examined a large number of my telepathy records for ± 1 displacement. In the case of Shackleton and of one other subject, I discovered highly significant evidence of such displacement on the $+1$ and -1 cards. It was not until much later that I examined more remote types of displacement up to ± 8 . On the $+2$ and $+3$ cards there were negative critical ratios of 2.97 and 2.05 respectively. These, by themselves, are not particularly remarkable, since they were chosen as the best of 13 tests. The 2.05 result could certainly be ignored.

But there is a most important consideration to be taken into account. In the 1936 work we were using largely Mr J. Aldred as agent whereas five years later, in 1941, we were working with a *different* agent, Miss Elliott. Now what is really extraordinary is that when we reverted again to J. Aldred in 1942, the -1 displacement which had been absent for a year with Miss Elliott suddenly re-appeared. It is not true, therefore, to say that only *one* of the positions tried in the former series gave significant matching in the later series. As is well known, the ± 1 displacement was consistently replaced by ± 2 displacement at the 'rapid' rate of calling.

A fairer way of putting the matter is to claim that in 1936 we discovered a *spectrum* of displacement scoring extending from -1 to $+2$, and thus confirmed the conjectures of Carington. Six years later, we obtained with the same agent, Aldred, a very similar spectrum. To demand that after *five years* the *exact* details of the displacement should remain unchanged is surely rather unreasonable. Actually the results produced by Shackleton in the 1941-3 series were consistently repeated week after week and month after month.

What Mr Brown does not mention is the very vital fact that in the Soal-Goldney experiments the results *varied consistently* when the experimental conditions were changed one at a time. I need only mention the total failure of the 'clairvoyance' tests and the *regular* shifts in the displacement when the rate of calling was doubled. No statistical artifact can explain such phenomena as these, any more than it can account for the personality effects observed by Humphrey, Schmeidler, and Nicol.

I have discussed the Shackleton experiments with many expert statisticians and I have yet to meet one who has even ventured to suggest that such significant scores, repeated week after week and

year after year, and in the presence of witnesses, could be attributed to any defects in probability theory.

In reply to Mr Brown's first letter to *Nature*, Dr Thouless, Professor Stratton and I pointed out that when a particular kind of control experiment, known now for twenty years as a 'cross-check', was applied to many of the high-scoring series, it invariably produced results which were quite insignificant compared with the original high scores. In a cross-check the percipient's guesses are compared with a pack of cards with which they were not originally associated *in any way*, and 'cross-check' was intended by us in this sense. But in his last letter to *Nature* (26 September) Mr Brown gives only examples of *other* kinds of control experiment which produced significant results. One of the examples he quotes—that of Coover—may be due to faulty experimental conditions. Coover did *not* obtain any significant deviation on his control but only on the control and experimental series *combined*. I pointed out, fifteen years ago, that this may have been due to the fact that Coover's *single* pack of cards which he employed may have cut most easily at certain popular cards such as aces, etc., and, if so, this might account for his very moderate extra-chance score.

If we refer to Nigel Richmond's paper we find that the author writes: 'A separate series of runs was also made... I have called these "control" runs to distinguish them from the "attempts", although perhaps the term is not entirely appropriate since there was a target (post-determined) for each card, and, as Dr Rhine has commented, the possibility was not excluded of the experimenter becoming aware of the card extra-sensorially and then applying the influence unconsciously either in a positive or a negative direction'. (*Jnl. S.P.R.*, xxxvi, 579). Thus Richmond did not carry out control runs from which psi phenomena were excluded, and Mr Brown's criticism is not appropriate.

Lastly, in the Forwald series, I have, at a recent conference, criticised the experimental conditions on the grounds that the proximity of the experimenter may have unconsciously biased the apparatus.

Mr Brown still seems unaware of the exact procedure adopted in a standard dice-throwing experiment since in his second letter to *Nature* (26 September) he asks if the target numbers are to be chosen by more dice throws. Actually, the subject tries in blocks of 24 trials for each of the six faces in turn, but the experimenter decides the order of the six faces by taking a row from a 6-6 Latin square. This simple method is adequate and is constantly employed in agricultural experiments with satisfactory results.

Even if it is ultimately agreed by statisticians that very occasionally two sets of 'random numbers' produce significant matchings of, say, the order of 3.3 standard deviations, this would be no argument against the reality of the results of ESP investigators, because many of these results are meaningful. Further, we should require to know how many experiments of the Spencer Brown type yielded only chance results. One result with odds corresponding to 3 standard deviations may cease to be significant if 30 other similar matchings give results which are all very close to chance expectation. Optional stopping is also an important factor in such experiments. That is, we must take into account the normal fluctuation of the critical ratio over a long series of trials.

S. G. SOAL

London, S.W. 12.

ANTOINE RICHARD'S GARDEN

SIR.—I think, like Mr Lambert, that the good faith of Miss Moberly and of Miss Jourdain cannot be called in question. I accordingly accept it as a fact that before their visit they had no detailed knowledge of the persons or objects at the Trianon, and that the narrative which they have left of their adventure must be considered absolutely sincere.

On this assumption, my opinion as a historian, after a thorough review of the documents we possess in the National Archives about the Petit Trianon—a task to which I have devoted some twenty years of my life—is that the verifications given by the two English ladies about the things which they saw are erroneous. It also strikes one that in identifying the persons seen with those mentioned in Julie Lavergne's book, published in 1878, they equally went astray. It is true that Julie Lavergne was in a position to gather the recollections of certain individuals who had lived at the Trianon, and had even been in the presence of the Queen, but to what extent could any credit be given to these little stories, charming as they are, which the author has not the temerity to describe as other than 'legends'? I have tried, for my part, with a completely impartial and objective outlook, to verify, by proofs which are both historical and irrefutable, the statements made by Miss Moberly and Miss Jourdain and my conclusion is as follows.

In the present state of my knowledge the historical check yields a negative result save on one point, which is apparently very small, but is really extremely important, namely the kiosk of which the roof presented 'a slightly Chinese effect'. Mr Lambert, and I am very flattered by his agreement, attaches, as I do, great importance

to this word CHINESE since, while challenging my suggestion that the kiosk is identical with the Jeu de Bague, he puts forward the hypothesis that the visitors must have seen the same vision of the Anglo-Chinese garden as that which old Richard had imagined; or, in other words, that the plan of the Queen's gardener was materialized before the eyes of Miss Moberly and her friend. And in fact I find that Miss Moberly's sketch, the reproduction of which was an excellent idea, resembles fairly closely, anyhow in the matter of size, that which Richard contemplated on the island. But before suggesting a new idea, I would like to reply to Mr Lambert's reasons for not recognizing the Jeu de Bague in the kiosk.

(1) The two ladies did not recognize the kiosk in the picture which they saw in the antique shop, representing the Jeu de Bague. But did that picture give a clear representation of Marie Antoinette's Jeu de Bague, or of that which Napoleon had made for Marie Louise, I think, on the same site? In the latter case, there would be no cause for surprise in their not recognizing it.

(2) *The surrounding*, which I understood in the plural sense. That does not seem to be a point of much importance.

(3) The kiosk did not occupy the same site as the Jeu de Bague. Mr Lambert says it was just *inside* the French Garden. It would be more accurate to say 'on the edge of the French Garden, between it and the English Garden'.

But please do not let us quarrel over these details. I merely persist in holding on to the word 'Chinese', and to the fact, of which I am absolutely certain, that outside the imagination of Richard there was never any Chinese structure at the Trianon other than the Jeu de Bague; but this, with the Gallery which encircled it, was so striking a feature as to cause the Duc de Cröy, as Mr Lambert so aptly recalls, to say when he was visiting the Queen's garden in 1782 that 'he found everywhere the English and the Chinese style'.

I think we shall find it difficult to establish by historical proofs, especially in detail, everything that the two ladies saw and heard, for the following reason. As I understand it, when we are talking of 'recognition' it does not follow that, because there is contact on one point, there must necessarily be contact on all other points. We know that in space disturbances occur which hinder or distort the transmission of waves. Why should not the same phenomenon be met with in that other dimension of the universe which is called 'time'? In connexion with the kiosk, one might thus explain the fact that the visual image which Miss Moberly and Miss Jourdain saw only agrees rather roughly with the past reality, especially as regards its situation, considering that the ladies found it further

off from the Queen's House than the Jeu de Bague was, and so on. That is, perhaps, a line of thought to follow up, but being neither a physicist nor a philosopher, I give it for what it is worth.

Whatever the cause may be, would that be a reason for giving up historical research? Certainly not. As I have said, I think I have examined all the archives of the Trianon, but it is still possible that in this or that series of the archives an unexpected document might be found throwing light on some hitherto obscure points. Thus, I do not at present know whether the gardeners at the Trianon wore livery and what colour it was, but perhaps some day I may obtain information on the point, and in that case I would pass it on.

I hope that my collaboration with Mr Lambert is not at an end, and I welcome it. I have read his study on Richard's Garden with much interest. In it he shows both learning and ability. With my congratulations, etc. [Translation.]

LEÓN REY

Paris.

Mr G. W. Lambert has furnished the following comments on the above.

In publishing my paper in the *Journal* for July-October 1953, at the incomplete stage of research then reached, it was a main object to enlist the help of others in the difficult task of historical elucidation. That help was not likely to be forthcoming unless a *prima facie* case had been made out. My first step, after the publication of the paper, was to send a copy of it to M. Léon Rey, the author of the article in the *Revue de Paris* of December 1952. I have been fortunate enough to engage his interest, for which I am extremely grateful.

I find myself in complete agreement with M. Rey's views both as to the good faith of Miss Moberly and Miss Jourdain, and as to the generally erroneous character of the interpretations they put upon their experiences. He finds nothing historically verifiable in their visions except the reference to a 'Chinese effect' in the roof of the kiosk.

M. Rey then goes on to deal with the three difficulties I raised about the identification of the kiosk with the Jeu de Bague. The fact, of which I was not aware, that there was a later Jeu under Napoleon, probably on the same site, and the possibility that the picture which the two ladies saw in the old print shop was of that, and not of the original Jeu, greatly weaken the force of my objection, and the same can be said of the second point. As to the third point, the difference in the position of the Jeu de Bague compared

with the kiosk, I accept M. Rey's very gently administered correction. I should, of course, have realized that the Jeu, a strange oriental novelty, by its nature belonged not to the French Garden, on the edge of which it stood, but to the English Garden, towards which it faced. Antoine Richard must have been greatly scandalized at its being placed on a site which almost encroached on the area of the French Garden. No doubt he wished it a long way off, out of sight and out of hearing.

M. Rey is convinced that, apart from the Jeu de Bague, there never was actually any other Chinese feature in the Garden. This confirms my first impression (p. 125) that there was little likelihood that a Chinese garden ever existed near the place where the kiosk was seen, but I was in some doubt owing to E. Rohde's inaccurate observation (see p. 124), and by the Duc de Cröy's too sweeping remark (see p. 142). Either the kiosk was simply a creature of imagination, or it was a displaced and distorted image of the Jeu de Bague.

Earlier critics took the line that the mental picture remembered some time after the experience was a displaced and distorted image of a building (or buildings) actually in the Garden at the time, and open to ordinary inspection. One might call this kind of alteration, due to tricks of memory and so forth, post-sensory distortion, which is, I think, a better description than 'retrospective falsification', the expression used by me on p. 138. M. Rey, on the other hand, suggests that the differences between the kiosk and the Jeu de Bague could be explained by a different process, which might, I suggest, be called, by contrast, pre-sensory distortion. He contemplates a persisting image of the Jeu in the 'time dimension', liable to distortion (both of position and of appearance) before it is perceived by a sensitive subject as a hallucination.

My answer would be that such a hypothesis is theoretically tenable, but is 'dangerous' if used to explain serious differences of place and shape. In the case of the kiosk, for instance, it would be easier to suppose that the slight flattening of the roof was itself due to pre-sensory distortion of the domed roof of the Temple of Love, with its circle of pillars, rather than of the middle portion of the non-existent Jeu de Bague, which had a single central support for the roof.

On similar grounds, I cannot myself believe that the kiosk, as shown in Miss Moberly's sketch, is a distorted image of the four-pillared kiosk contemplated by Richard for Site 7 in his Plan. The two may be similar in size, but their styles are entirely different.

In addition to the comments in the foregoing letter, M. Rey has very kindly furnished me with some information on points of detail, of which I will make use, with due acknowledgement, in a future paper giving the results of further research.

'PHANTASMS OF THE LIVING AND OF THE DEAD'

SIR,—The article on 'Phantasms of the Living and of the Dead', by Professor C. D. Broad, in the May 1953 number of the *Proceedings* is a highly stimulating discussion of a fundamentally important problem. Professor Broad's eminent distinction—both in philosophy and in the field of parapsychology—lends weight to the generalizations which he presents, and support to the arguments which he offers.

In addition to this authoritative character of his writing, Professor Broad frequently intimates that his statements are inductive conclusions from systematic data. If articles in our proceedings are to promote accurate and factually supported thinking, collectively and co-operatively developed by the participation of members who (as in my own case) are not always fully acquainted with all of the cases which have been published in the literature, it seems desirable that the references to supporting facts shall be at least sufficiently explicit to enable the thoughtful and critical reader to go back to the purportedly supporting evidence.

For example, on page 62 Professor Broad says: '(ii) We have some reason to believe that the *details*¹ of a telepathically initiated hallucination are largely the work of the *recipient* of the influence.' Presumably this generalization may refer to Tyrrell's study of apparitions.² If so, it would help if the specific references were given. If Professor Broad has other evidence of this conclusion than Mr Tyrrell's discussion, citations of his data would aid those who seek to follow his thought intelligently.

Again, on page 60, Professor Broad says: '... there is evidence that a telepathically initiated stimulus may not give rise to a conscious experience until some time after the event which initiated it.' That conclusion appears to have played a central part in the logical analysis contained in *Phantasms of the Living*. However, explicit and clear-cut conclusions in such a discussion as this might be promoted by a specific reference to at least the outstandingly best summary of the evidence which has led to this (may I say?) at least debatable conclusion.

¹ In all passages quoted the italics are Professor Broad's.

² G. N. M. Tyrrell, *Apparitions*, Seventh Myers Memorial Lecture, 1942. (New edition: London, Duckworth, 1953).

Similar needs for specific references to either original case material or to systematic studies based on such material seems to the present writer to be apparent in the following places :

On page 53 : 'I should class these two alternatives together as instances of a *collective hallucinatory quasi-perception*. There is very good evidence for cases of this kind.' Where?

On page 54 : 'There is often no good evidence that their hallucinations were so correlated as they would be if they were actual perceptions of the same real object from various points of view or at various stages of its history.' In what specific cases has this lack of correlation been made evident?

On page 58 : 'Or (ii) the details of *A*'s hallucination might correspond not to *B*'s actual state, situation, dress, etc., at the time, but to the contents of *B*'s contemporary *false beliefs, delusive hallucinations*, etc., concerning these matters.' In what specific cases has this been observed, or what specific study has brought together the evidence supporting this conclusion?

On page 61, paragraph (2), Professor Broad describes in general 'some very impressive and well-attested cases of the following kind.' Where are those cases located?

Later on page 61 : 'When one studies the details of the best attested cases of "haunting" they do not, I think, on the whole suggest the presence of any persistent desire or intention.' Specific citations of some of the 'best attested cases' referred to here would aid the critical reader at this point.

On pages 56 to 57, Broad gives a summary of the famous Wilmot case, disguised under the pseudonyms 'Mrs *A*' and 'Mr *B*'. In the next paragraph he similarly disguises a case of hypnotic ESP projection, which I do not recognize but should like to locate. Why not state these cases in actual terms, with references to sources, rather than in anonymous disguise?

The above suggestions all relate to the need which the present reader feels for specific factual annotation of the exceedingly interesting and stimulating generalizations which Professor Broad offers.

HORNELL HART

Duke University,
Durham, North Carolina, U.S.A.

SIR,—Professor Hornell Hart's friendly and appreciative letter of admonition, dated 28 September 1953, calls for a brief answer from me.

I am in principle fully in sympathy with his plea that detailed

references to relevant literature should be supplied by authors who theorise on the basis of alleged observations and generalisations from them. I regret that I can do little here and now to supply the references which he would like to have, since I am at present out of England and out of touch with the literature of psychical research, and this state of affairs will last for the next nine months or so.

I will, however, make the following explanatory comments, in view of Professor Hornell Hart's letter :

(1) The article in question was written as a lecture to be given to a non-specialist audience. It was one of a series of lectures given by several members of the S.P.R. when the Society reached its seventieth year. In the original lecture elaborate references would have been quite out of place, but I admit that they might with advantage have been supplied when the manuscript came to be printed as an article in *Proceedings*.

(2) I would call attention to the fact that I was explicitly discussing topics treated in two classics of psychical research, viz. the book *Phantasms of the Living* by Gurney, Myers, and Podmore, and the paper *Phantasms of the Dead* by Mrs Sidgwick. Except where other works are explicitly referred to, the cases in question, and the generalisations, are all contained in these two publications.

(3) In some of his references Professor Hornell Hart has ascribed to me statements which I explicitly made and discussed as paraphrases of Gurney's statements. This is quite plain in the passage quoted from p. 62. It is obvious from the context there that I am not expressing my own opinions, and that any reference to Tyrrell's Myers Memorial Lecture would be an anachronism. Similar remarks apply to the passage quoted from p. 60, where the reference to the authors of *Phantasms of the Living* is explicitly made.

(4) I think that anyone who consults *Phantasms of the Living* and *Phantasms of the Dead* will have no difficulty in finding case-material in support of the statements quoted from p. 53 and the two quoted from p. 61. But it would no doubt be highly desirable to go through the later literature in order to see whether subsequently reported cases had provided further confirmation of the older generalisations, or counter-instances to them, or had simply failed to supply relevant evidence one way or the other.

(5) The plea which Professor Hornell Hart makes after his quotation from p. 58 seems to rest on a misunderstanding. I was not claiming that hallucinations of the kind there described as possible do in fact happen. I do not know whether they do or not, and I should think that it would be difficult to find conclusive

evidence for them if they do. But that is irrelevant for my purpose, which was simply to point out and distinguish two alternative possibilities covered by the phrase 'veridical telesthesia'.

C. D. BROAD

University of Michigan,
Ann Arbor, Michigan, U.S.A.

ESP, SPACE, AND TIME

SIR,—I should like to comment on the correspondence printed on pp. 81, 180, and 214 of the present volume of this Journal, especially on the final paragraph of Mr Smith's letter (p. 180). It is my opinion that, as research at present stands, we can hazard no judgement on any connexion—or lack of one—between ESP and space or time.

It is obviously useless, as Mr Smith realizes, to compare results obtained at different distances if they are from different experiments. Because B.S. in Dr Soal's experiments (*Proc. S.P.R.*, Vols. 37 and 46) obtained significant results when a few yards from the agent, and Whately Carington's picture series of experiments with percipients scattered throughout the country (*Proc. S.P.R.*, Vol. 46, pp. 34-151 and 277-344) were also successful, we are not justified in saying that ESP is independent of space. To draw quantitative inferences about the effect of distance, it is clear we need a series of experiments with identical agents, percipients, experimenters, and conditions, conducted over varying distances. Then we may think about comparison of results; but we have still not eliminated the notoriously uncontrollable psychological variables. The observed results could be due to chronological decline, inconsistency of scoring level,¹ the experimenter's influence² or the theories he holds, emotional disturbances in the percipients, or to a hundred and one psychological reasons.

With spontaneous cases, we can explain why, as Mr Smith notes, dreams fulfilled within a shorter period appear to outnumber those fulfilled within a longer one. Many precognitions are of trivial events which, if not rapidly fulfilled, are forgotten. Even if a serious event such as a death is precognized many years in advance, it has to contain striking detail, or be repeated (as was the well-known one of Lady Q in *Proc. S.P.R.*, Vol. 11, p. 577) before notice is taken of it. If I dream of a death in perfectly ordinary circumstances and without a wealth of detail, and ten, twenty or

¹ Or 'perspicacity' as Mr Chesters calls it (p. 215).

² See, for example, the comments by D. J. West and G. W. Fisk on pp. 23-4 and 185-9 of this Volume of the Journal.

more years later, the death occurs, do I immediately and enthusiastically communicate the fact to the S.P.R.? It is unlikely ; but if the death takes place the next day, I may be inclined to do so.

Hence I think that, even for terrestrial distances, and times within a life-span, we are as yet unjustified in making any assumptions as to whether ESP is affected by space or time.

MICHAEL C. PERRY

Trinity College, Cambridge.

THE PERROTT STUDENTSHIP IN PSYCHICAL RESEARCH AT TRINITY COLLEGE, CAMBRIDGE

THE following communication has been received from Dr C. F. A. Pantin, Trinity College, Cambridge :

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined for the purpose of the Studentship as 'the investigation of mental or physical phenomena which seem *prima facie* to suggest (a) the existence of supernatural powers of cognition or action in human beings in their present life, or (b) the persistence of the human mind after bodily death'.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating some problem in Psychical Research. The Student shall not, during the tenure of his Studentship, engage in any other occupation to such an extent as would in the opinion of the Electors interfere with his course of research.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake. The emolument will, in general, be paid half-yearly, and the first instalment will be paid on the quarter-day on which the tenure of the Studentship begins.

The Student shall, during the tenure of his Studentship, pursue to the satisfaction of the Electors the course of research proposed by him in his application ; provided that such course may be altered with the consent of the Electors. The Electors will appoint a Supervisor with whom the Student is to keep in regular touch. If the Electors shall report to the Council of Trinity College, Cambridge, that the Student is failing to pursue his course of research with due diligence, the Council may, if they think fit, deprive him of his Studentship.

Applications from candidates should be sent to The Secretary,

Perrott Studentship Electors, Trinity College, Cambridge, before 30th April, 1954. In making his application, a candidate should state his qualifications and claims, and his proposed course of research ; he may also submit any work which he has written, published or unpublished. No testimonials are required from candidates who are graduates of Cambridge University. Other candidates must submit the names of three referees, and the Electors will not award the Studentship to any such candidate until they have had a personal interview with him.

The election to the Studentship will take place in the Easter Term of 1954, and if a candidate be elected his tenure will begin at Michaelmas following the election.

The Electors are Dr Pantin, Dr R. H. Thouless, and Professor A. L. Hodgkin, F.R.S. The previous holders of the Studentship have been the late W. Whately Carington, Dr S. G. Soal, and D. G. Spencer Brown.